THE ASTRONOMICAL AND ASTROPHYSICAL SOCIETY OF AMERICA.

II.

Photographs of Nebulæ and Star Clusters obtained with the Crossley Reflector: By J. E. KEELER.

The photographs on glass were exhibited by Professor Campbell, who called attention to some of the difficulties which had to be overcome in the use of the instrument and to the important results established, that nebulæ as a rule are spiral in form and as an exception irregular in form.

Some New Spectroscopic Binaries: By W. W. CAMPBELL.

A list of half a dozen spectroscopic binary stars, recently discovered by Messrs. Campbell and Wright at the Lick Observatory. (To be published in the *Astrophysical Journal.*)

A Simple Method of inserting the Comparison Spectrum in Spectrographic Observations: By W. H. WRIGHT. Read by PROFESSOR CAMPBELL.

The length of the slit is limited by two total reflection prisms mounted immediately in front of the slit plate. The reflecting edges of the two prisms are parallel to each other and at right angles to the slit; and the distance between the reflecting edges is the length of the slit. The artificial light, from two sets of electrodes placed one near each end of the full-length slit, enters the slit through these prisms near their vertices. The method possesses many advantages. (To be published in the *Astrophysical Journal.*)

Color Screens as applied to Achromatic Telescopes: By GEORGE H. PETERS.

Attention was called to the want of definition caused by the secondary spectrum in large refractors. It was then shown how this was removed at the U. S. Naval Observatory by adapting a cell filled with an absorbing solution between the eye-piece and eye of the observer in the 26-inch telescope. The thickness of the absorbing layer is about 6 mm., and the solutions used are the following:

1. Picric Acid and Chloride of Copper dissolved either in water or alcohol, for cutting out the red and blue light.
2. Picric Acid dissolved in alcohol or water.
3. Bichromate of Potash in water.
4. Chromate of Potash in water.

Solutions 2, 3 and 4 remove the blue and violet light. Alcohol is used in cold weather. The absorption of the liquids is tested by a direct vision spectroscope.

Some of the benefits derived from the color screen are:

Improved definition on planets and satellites and in certain classes of double stars.

In observations of objects by day where the blue light from the sky is removed.

Professor See and Mr. Peters are jointly credited with this device. (See *Astronomische Nachrichten*, No. 3636.)

The Spectrographic Determination of the Motions of the Stars in the Line of Sight: By W. W. CAMPBELL.

Complying with the Council's request for a paper on this subject, the following points were presented:

The great interest taken in this problem is attested by the fact that the largest refracting telescopes in the United States, France, England, Germany, Russia and Africa are, or soon will be, largely devoted to the determination of stellar velocities.

It must not be assumed that, because a 36-inch objective receives nine times as much light as a 12-inch, it will be able to carry velocity determinations in that ratio to the fainter stars. The larger telescope is accompanied by stronger absorption of the light, by larger stellar images (except in perfect seeing), by greater difficulties in guiding, etc.; so that it is able to extend the

work of a 12-inch telescope only 5- to 7-fold. A 36-inch reflector, suitably mounted, and in a dry climate, would probably be more efficient than a 36-inch refractor in this problem.

Success in determining stellar velocities depends upon a great many elements, of which the following were all that time permitted a mention.

I. A suitable compromise between the various optical and mechanical features—many of which conflict—to secure a powerful and efficient spectrograph.

II. Suitable prisms and lenses. The requirements demanded by the line of sight tests are so severe that satisfactory lenses and prisms are difficult to obtain. Since a change to a new prism-train usually requires many changes in the mounting, the prisms should be constructed and thoroughly tested before the rest of the instrument is designed.

The day for using the ordinary double camera lens is past. Even the triple lenses do not fulfill the difficult spectrograph's requirements.

III. Absence of differential flexure during the exposure time: the optical angles must remain constant. This is very largely a problem in mechanical design. The elimination of appreciable flexure effects during long exposures is difficult, but not impossible. Existing spectrographs, including those just now completed, are wrongly designed, in that they are supported only at one end. The heavy prism-end projects out into space, unsupported. The spectrograph is held out 'at arm's length,' so to speak. Now the spectrograph is an instrument complete in itself, and need not be rigidly connected with the telescope tube. A framework connected with the telescope could be arranged to support the spectrograph, in position, both near its upper and its lower ends; care being taken that strains in the supporting framework should not be communicated to the spectrograph. This form of support would permit a more economical distribution of material in the spectrograph, and should decrease the flexure effects several fold.

IV. The Mill's spectrograph has been enclosed in a wooden box case, lined with thick hair felt, carrying on its interior surface about 30 feet of German silver wire. As the temperature within the case falls, a storage battery is connected with the resistance wire. The heat generated tends to keep the instrument, and especially the prisms, at a constant temperature—a most important desideratum.

V. An accurate method of guiding during the exposure.

VI. The use of the finer dark lines throughout the spectrum, rather than of the heavier lines which have heretofore been the ones measured.

VII. Long experience in making the measures.

An exposure of one hour is required for a star of the 5th photographic magnitude, as given in the Draper Catalogue.

At the Lick Observatory two or more plates have been secured for each of about 300 stars. For the best stars, such as *Polaris*, the probable error of a single determination of velocity is about one-third of a kilometer per second. For such stars the plates seem to be essentially perfect, and to secure greater accuracy it appears that a more powerful spectrograph is required.

Even with perfect plates the observer must be on his guard against systematic errors arising from changes in his personal habit of measurement.

Of the 300 stars observed, 22 have been found at the Lick Observatory to be binary, in addition to three previously detected from the same list by Belopolsky. This leads us to the important conclusion that one star in twelve is a spectroscopic binary. And it is probable that others of the stars observed are binaries as yet undetected.

Some Peculiarities in the Radial Velocities of ξ Geminorum: By W. W. CAMPBELL.

This is a well known variable star, period of $10\frac{1}{6}$ days. Its velocity in the line of sight was found, independently at Pulkowa and Mt. Hamilton, to be variable. Forty-five observations at Mt. Hamilton, distributed over $1\frac{1}{2}$ years, left no doubt that this star is a spectroscopic binary, and that the variation in brightness is produced by the companion. The eclipse theory is untenable. The variation is probably the result of tidal action between the two bodies.

The observed velocities cannot be represented by elliptic motion. Drawing the elliptic curve which best represents the observations, the observed curve is alternately above and below it, crossing it at intervals of about 41 hours. Three complete periods of the minor irregularities coincide with one light period. These minor irregularities in the observed velocities are probably due to minor tidal effects in the star's atmosphere. (To be published in the *Astrophysical Journal*).

H. M. Paul. I would like to ask if these observations were plotted to represent the total amount of observations, obtaining the period from these observations.

Mr. Campbell. They were all plotted with reference to the brightness of the star assuming the old period. There is some doubt as to the exact form of the light-curve. I have endeavored in every possible way to ascertain the form of the light-curve; but there seem to be some irregularities, and I think that extremely accurate observations would show that the brightness curve has the same irregularities as the velocity curve; that there is a reflex action of one upon the other.

G. W. Hough. Were there any systematic observations to ascertain the cause of irregularities with reference to motion in the line of sight?

Mr. Campbell. No; we have obtained our results merely as by-products from the system of guiding that we have employed; and we have made changes in the system of guiding in the expectation that if we could eliminate the difficulties arising from that, so as to make it satisfactory for one bright star, it would be satisfactory for two stars.

Mr. L. E. Jewell. Mr. Campbell has suggested that the irregularity in the motion in the line of sight may be due to tidal action, which I think is probably the case; but he has also suggested that it may be due to tidal compression, which I do not think can explain the phenomenon. I have myself been engaged in some work bearing upon that problem, and among other things the pressure of the different layers of the solar atmosphere; but the lines which have been measured in the solar atmosphere do not have a pressure of more than $1\frac{1}{2}$ atmospheres, considering the pressure of the earth's atmosphere as one. The variation due to difference of pressure would be too slight to produce the effect that he speaks of. I think it probable some tidal action causes the motion in the line of sight, and that it is not due to change of pressure, because the differences of pressure would be too slight to produce such an effect. In the case of the sodium lines in the sun's atmosphere, the actual pressure is only about an atmosphere and a quarter. The variation in the hydrogen lines would be very small, and it is probably produced in a higher layer of the sun's atmosphere.

Mr. Campbell. We seem to have no analogous case to this star from which to obtain data to explain it. We have no knowledge of tidal action elsewhere, but here we seem to have enormous evidence of tidal action.

T. J. J. See. I think the explanation of the cause of the second curve superposed upon the first curve is very probable. In the case of the ocean tides, we have very little vertical motion and a horizontal

motion many times greater. The distortion shown in the figure indicates that the horizontal motion is very large.

W. J. Humphreys. Some work has been done by Wilssing at the Potsdam observatory, showing a great change from change of pressure; but he applies this to such stars as Nova Aurigae. He has obtained results, working with hydrogen, showing that the spectrum lines in hydrogen are very sensitive to diminution of pressure. This applies especially to the deeper lines. He has obtained perfectly sharp lines with a pressure of 14 atmospheres.

Mr. Jewell. From our knowledge of the solar spectrum it is perfectly certain that the lines do not show any evidence of powerful pressure. Under such pressure the lines would probably be broader.

The Indian Eclipse Photographs of Jan. 22, 1898, obtained by the Lick Observatory Expedition : By W. W. Campbell.

Photographs of the corona and its spectrum; and of the spectrum of the sun's edge, were thrown on the screen. Special attention was called to the apparent and probable connection between the prominences and the curved coronal streamers enclosing them and to the relative displacement of the bright and dark lines in the spectrum of the sun's edge.

Total Solar Eclipse of May 28, 1900.

Simon Newcomb presented the report of the committee upon the observations of the recent eclipse.

W. W. Campbell. Being a long distance from our base in California, it was impossible to take our plates home for development, and they were developed in camp, where it was impossible to do photographic work successfully. I have brought one of the original negatives, but Mr. Brown should have the privilege of presenting it. It was taken with an exposure of one second, with a 40-foot camera. It reveals the fact that the coronal tails were not so numerous as those in 1898, while those were not so numerous as in 1893. There seems at first to be no connection between the coronal streamers and the prominences; and yet, if you will go over the plate carefully, I think you will see there is some connection between them. Our spectroscopic material was not so great as we should have liked to obtain; but for the third contact we obtained very satisfactory results. It was a question whether bright lines would be shifted towards the red end, but they seem to show no evidence of such shifting.

S. J. Brown. The eclipse observations of the Naval Observatory were widely distributed and covered a great deal of ground. Two main stations were occupied, one in North Carolina and the other in Georgia. Our photographic work is not in a condition to exhibit. There was one experimental line of research I think it worth while to mention. We wished to try experiments with color screens; and with one lens we used isochromatic plates, and with another lens the ordinary photographic plates, to see whether any indications would be shown as to a difference of structure of the corona due to the green line. The result showed no difference of structure, but there seems to have been a greater expansion of the polar rays when we used the color screens than when we used the ordinary plates. Photographs were taken to obtain as great an extension of the corona as possible; but the sky was not favorable, and we did not obtain so great an extension as was shown by the results in India. The spectroscopic work occupied our most serious attention. We had some very rare gratings, ruled to concentrate the light of the first order at a special point. Two of these gratings, from the Johns Hopkins University, were utilized, one at Pinehurst and one at Griffin, besides another grating of long focal length. Our preparations for this

work were much hurried, but we had gratings of which the curves were carefully computed for the focal length of the quartz lenses. At Griffin we obtained a reversing layer of eight seconds. Mr. Jewell has brought some copies of the only successful plates at Pinehurst, showing several new coronal lines, one coinciding with the 1474 lines, and there were three other new lines. Dr. Dorsey of the Johns Hopkins University also took photographs, and Dr. Wood of the University of Wisconsin undertook to study the shadow bands which precede and follow the eclipse. He is not here, but he thinks he has reached a satisfactory conclusion with regard to the cause of the shadow bands. While our results in spectroscopic work were disappointing, in view of the large outlay, they seem to vindicate the value of that method. We think we shall be able to obtain a plate which will bend to the required curvature, and thus be able to photograph the reversing layer.

L. E. Jewell. We obtained a different wave length for the bright and for the corresponding dark lines; and I think that difference in wave length is caused by the fact that the dark lines were produced very close to the moon's edge, by the remaining portion of the crescent, not covered by the moon, while the bright lines were produced further out. On one of the photographs taken at Pinehurst there is a very distinct difference of the *H* and *K* lines, between the bright and dark portions of the spectrum. The bright line is displaced with reference to the dark line in the direction outside, or further away, from the sun itself, just as it should be if that were the true explanation. In determining the method we should follow in the use of these plates we were largely influenced by the results of Evershed in India in 1898, showing that the chromosphere and reversing layer were much brighter in the upper part, showing other interesting phenomena. Another interesting thing was the presence of the rare element scandium. It was thought best to use his grating for the purpose of our work, and I made comparisons showing that he used a dispersion about three times that of a single prism; and we concluded that we could obtain at least as much light from our grating as from a prism. Our results at Pinehurst were thought of extreme importance. With an exposure of only one second we obtained results showing that if we had been able to bend the plate to the required focal length, it would have given us a spectrum from wave length 30 to 60, giving us 1300 lines; the effect being very sharp lines in the neighborhood of the *H* and *K* calcium lines, and pictures of the chromosphere proper and the reversing layer showing 50 to 75 prominences. The difference between the calcium and the hydrogen lines was shown very prominently indeed. A plate out of focus showed the yellow sodium line, and the coronal lines at 1474 which were seen much stronger on the opposite side of the sun from that where the ordinary bright lines are seen. This green line was much brighter than on the western side of the sun. There is no line which seems to be evenly distributed on the two sides of the sun. The green line seems to be gathered into a very short arc on the western limb. On another plate taken 25 seconds later, the exposure was 5 or 6 seconds, and on that these coronal lines are shown all the way round. There are four lines not obtained before, the substance producing them seeming to be about the same as that giving the green lines. They show the strongest at some distance from the sun's surface, say three to five thousand miles above the solar surface, although they are visible close to the solar surface. Another interesting thing was the beautiful way in which the carbon band showed with an exposure of one second. It was very strong a hundred

miles from the solar surface. One word with reference to future work: I think it is very desirable to use a concave grating in the ordinary way, but instead of using a dense lens of quartz, to use a very large concave reflector of a focal length of 100 to 150 inches, which would give an image of the sun an inch and a half in diameter; and then we could use a wide open slit so as to be certain to get the limb of the sun inside of it. At Pinehurst we had to cut down the slit in consequence of the curvature, and had an image of the sun only a third of an inch in diameter to work with. In future work much the best plan will be to use two concave mirrors, one of a short focus for the dense lines, and the other of a considerable focal length, the slit being placed between them. In this way we can let the light come through the narrow slit, and then the larger and long focus mirror will act as a condenser, and there will be no disturbance whatever in the focus. A telescope of high power should be used, and focused very carefully; and the light reflected from the second mirror would give us parallel light; and if the adjustments were well made the image would be very sharp. By that method we ought to be able to obtain very satisfactory results.

Winslow Upton. I will report briefly the work we did near Norfolk by very ordinary astronomical and photographic processes. We had a four-inch telescope for visual work; and we had an equatorial stand upon which a camera was carried by clockwork. The camera contained two very long focus lenses. We also had four ordinary cameras, which we used for the purpose of obtaining the extension of the corona. We found that the duration of the eclipse was shorter than calculated. By the English and the American Ephemeris it was 101 seconds, while we made it 99 seconds. In the visual observations we used a magnifying power of 50 or 60. At the time I did not notice the hooded arrangement of the corona, this gigantic hood overlapping the strata, shown in the photographs of the eclipse of 1878. In our observations to determine the place of the moon, and in the search for an intra-Mercurial planet, we carried out the instructions of Professor Pickering, which were to expose for 60 seconds. Our cameras covered a range about 25° along the sun's equator, and with 8 or 10 gigantic plates we covered that region; but we got nothing except one or two bright objects, possibly only the planet Mercury, and there seemed to be star trails instead of images, perhaps from a fourth magnitude star. I think the exposure might have been longer; for I feel confident that we could have exposed 80 or 90 seconds without fogging the plate. For the spectroscopic work we used triple-coated plates, especially made for us by the Seed Co., and also orthochromatic plates. With regard to the development of the plates we decided to vary from the instructions of the Committee. The instructions were that we should use an exceedingly weak developer and develop a long time. Professor Pickering on the other hand recommended a strong developer, and a great deal of bromide. As we had duplicate plates we decided to try both plans, and I think on the whole that we obtained better lines with the strong developer and the bromide restrainer. I have here some lantern slides which I will show.

Ormond Stone. The University of Virginia took part in the eclipse observations at Waynesborough, N. C. We used a 40-foot camera. I desire here to express my gratitude to Professor Campbell for the advice he gave us at the beginning, and for his kindness in assisting in the preparations, and also in developing our plates for us. Our preparations were begun very early and we went to the camp about a month before the eclipse took place. At the time of the eclipse the sky was perfectly clear,

and we took a very good series ef pictures, showing a vast amount of detail clear down to the sun. We have learned much of what ought to be done in the future. Eclipse problems are constantly changing and new methods are used. In the adjustment of the frame in position, I think it ought to be done by actual observation of the stars, rather than by experiment. We happened to get our carriage some inches out of place, but by our observations we brought it within 1–16th of an inch of its true place. We may study the photographs as to the forms of the filaments, and the relations of the chromosphere to the protuberances; but that ought to be supplemented by visual observations with the telescope. A six-inch telescope with a magnifying power of 50 will show us ten times as much as can be seen in the best photographs. But the observer sees it and no one else, so that we still need the photographs. The plates were exposed by Mr. Mayo, who had himself constructed the apparatus. Mr. Morgan had a four-inch equatorial, and I used a six-inch equatorial and observed visually. It was our purpose to examine each of the prominences in succession to see if we could perceive any distortion. We could see no relation to the protuberances, and our visual observations simply confirmed what was shown in the photographs. At the eclipse of 1878 the filaments were well seen, and each one stood out distinctly. At this eclipse there was not that sharpness, and there seemed to be a strong background of something else to the equatorial filaments. In 1878 at the poles the rays were very much as in this eclipse. In the equatorial region there was a perfect network of filaments. They started out and moved right back with an incurvature in the opposite direction, so that there were a number of filaments side by side. They are certainly not hyperbolic curves, but we do not know what they are. One suggestion is that they are meteoric or comet streams. Our exposures were mostly short; we had nothing exceeding 12 seconds. It would seem well to have some longer exposures. I am glad the Naval Observatory has secured a plate with a longer exposure, on which we can see longer filaments and learn more of the nature of the curve. By tracing the curve back to the sun's surface we may be able to get some idea of its form. We had a number of small cameras mounted on a polar axis and managed by Mr. Lyon, and with these we used a color screen such that the green line was not seen, but on an open space. I think in another eclipse we ought to get more color screen pictures with our large cameras.

Mr. C. C. Abbott presented a report of observations at Waynesborough.

Mr. Dorsey explained the mode of determining the polarization of the bright coronal lines.

The Secretary read the following reports from Edwin A. Frost and E. E. Barnard upon their observations of the eclipse made at Wadesboro, N. C.

E. B. Frost. Four spectroscopes were employed: 1. A small plane grating in front of a lens of 2-inch aperture and 20-inch focus, for visual use in determining proper instant for exposure for obtaining 'flash' spectra.

2. Train of 3 large flint prisms, at minimum deviation for λ 4230, in front of photographic lens of $3\frac{1}{2}$-inch aperture and 42-inch focus; for photographing flashes and cusps and coronal spectra.

3. Small concave grating used directly, without slit; focus 30-inch for photographing flash spectra.

4. Large plane grating of 20,000 lines to inch, in front of visual lens of $3\frac{1}{2}$-inch aperture, 42-inch focus, with red screen, for photographing red end of spectrum of cusps, flash and corona. All placed in horizontal beam 13-inch diameter reflected from

cœlostat. Time called by Professor A. S. Flint; exposure of 2 and 3 operated by Dr. G. S. Isham of Chicago; signal for exposure given by visual observation of flash with 1 by E. B. F., who operated 4.

Results: 1. The flash spectrum was visually observed in the yellow near D_3 at second and third contacts. The number of lines seen, however, was not as large as might have been expected; some 12 or 15 were seen in the small field employed. These were broken by the prominences, and were doubtless chiefly lines of the usual chromospheric spectrum. No attempts at measurements were made, as the object of the visual observations was to find the instant for making the exposures for flash spectra. The dark lines of the cusp spectrum were sharp and distinct 45 seconds before the computed time of totality. A glance into the eyepiece at the middle of totality showed no coronal rings in the field, which did not include the region of 1474 K.

2. Objective train of three large prisms (mean path of the rays in the glass was 8 inches). Five exposures were made, on two plates, all of which were successful.

(*a*) The two spectra of the cusps, taken before and after totality, show many bright (chromospheric) lines in addition to the dark lines. The region covered by the plate was necessarily limited, by the construction of the plate holder, and includes only from λ 4380 to λ 4025. Fifty bright lines were measured on the second cusp spectrum.

(*b*) The first flash spectrum shows no continuous spectrum, but will furnish the wave-length of many bright lines. Something over 100 lines appear to be measurable within the above limits.

The second flash spectrum was overlaid by a band of solar spectrum, the third contact occurring some seven seconds earlier than was expected and indicated by the count. This plate, however, shows a large number of bright lines, 275 having been measured on it by the writer between λ 4380 and λ 4027. Young's list of chromospheric lines contains 27 in that region.

(*c*) The coronal exposure was of 30 seconds, but could have been longer to advantage. The rings of $H\gamma$ and $H\delta$ (chromospheric) are strongly impressed, showing the prominences, and next in intensity is the strong chromospheric rings of wave-length λ 4078. Four other rings are seen in whole or in part, the coronal ring at λ 4231 being next in intensity, although faint. It shows none of the broken appearance which characterizes the chromospheric rings. Another ring of longer wave-length than the $H\gamma$ may also prove to be coronal. The continuous spectrum of the corona is quite strong. This plate has not yet been accurately measured.

3. The small concave grating, without slit, was used for first and second flash.

On the first some 70 lines (bright) are shown between $H\beta$ and $H\delta$; on the second about 110 in that region, with about 50 others less sharply in focus, chiefly on the violet side of $H\delta$.

4. With the limited time for exposure, the sensitiveness of the plates, of the 'Erythro' brand was insufficient to record the flash spectra. The plate exposed for 60^s to the corona shows no impression, as was indeed expected. The spectrum of the cusp ten seconds after totality shows some dark lines and a few bright lines, and the second flash spectrum is seen in traces.

E. E. Barnard. The following preliminary statement concerns the photographs of the corona obtained by the Yerkes Observatory expedition to observe the total eclipse of the sun at Wadesboro', North Carolina, May 28, 1900.

The weather conditions were as near perfect as possible.

The most important instrument was a $61\frac{1}{2}$-foot horizontal cœlostat with an excellent 6-inch photographic objective made

specially for the purpose by Brashear. The light was thrown into the telescope by a very perfect plane silvered mirror made by Mr. Ritchey for the instrument. The equatorial mounting that carried this mirror was also made under Mr. Ritchey's direction though not for this special purpose. It was, hower, adapted by him for a 48-hour revolution. This instrument was driven by the clock from Professor Hale's 12-inch equatorial. The mounting also carried a 15-inch mirror for Professor Frost's spectroscopic work.

A long horizontal tube connected the 6-inch objective with the photographic house. This tube consisted of a wooden frame work covered with red water proof paper and was whitewashed on the upper part. A white cloth covering was also placed over it with a few inches of air space between the cloth and the tube. Frequent diaphragms were put along the inside of the tube to prevent stray light or reflections.

The photographic house in which the plates were to be exposed, was about 30 feet by 6 feet; it was of wood made thoroughly light proof by heavy red paper.

The plates were placed in a carrier 15 feet long, which ran on ball bearings in a light-tight sheath—the exposures being made through an aperture in the sheath where the image fell.

With this carrier, by rapidly shoving it along, one plate after another could be substituted for exposure with the minimum loss of time, about four seconds for a change. With it seven plates were exposed. Three of these were 14x17 inches, and the other four 25x30 inches. The following exposures were given these plates.

No.	Exposure.	Size of plate.	Kind of plate.
1	$\frac{1}{2}^{s}$	14 x 17	Cramer double-coated.
2	2	14 x 17	Cramer 'crown.'
3	8	25 x 30	" "
4	30	25 x 30	" "
5	14	25 x 30	" "
6	4	25 x 30	" "
7	1	14 x 17	Cramer double-coated.

A stop, regulated by hand, fixed the position of the carrier at each exposure, so that the image should always fall central on the plate. The plates were all set parallel to the celestial equator.

The exposures were made by a wooden exposing shutter, with a round aperture on one end of it. This shutter was controlled by two cords, one from each end, running into the photographic house. These cords being held taut, one in each hand, the exposures were made with absolute certainty and rapidity, by a quick pull with one or the other hand.

In the actual work, the carrier containing the plates was moved forwards by Mr. Ritchey after each exposure. At his signal 'ready' the proper exposure was given by me, the time being counted from a sounder beating seconds.

The seven plates were exposed without a hitch, the program being successfully carried out.

The signal for the beginning of totality was given by Mr. Putnam of the U. S. Coast Survey, who observed the contacts with a telescope at the Smithsonian Station, a hundred feet or so distant.

The exposures were begun immediately upon the signal for totality. A signal was also to be given ten seconds before the close of totality. This last signal was heard while preparing for the last exposure, and certainly within three or four seconds of this the exposure was made; the sun was then just appearing as a small speck of light—the end of totality having apparently come sooner than was expected.

At each exposure the image was seen on the plate and I was at once impressed with the striking likeness it bore to the corona of January 1, 1889. Though the moon appeared very black on the plates, the corona itself was disappointingly feeble. In the casual inspection, during exposure, the prominences were not noticed and but little detail was visible.

On the night before the eclipse each plate had been carefully backed with a non-halation solution consisting of caramel and burnt sienna. The plates were at once placed in the carrier ready for exposure.

In preparing for the eclipse the question of sensitiveness of the plates to be used was carefully discussed with Professor Hale and we finally decided to use the most sensitive plates. The result justifies our decision and shows that with a long focus instrument like this ($\frac{1}{123}$) the plates cannot be too sensitive.

Two of the plates were Cramer double-coated and were supposed not to be so sensitive as the Cramer 'Crown.' Plate No. 1 (double-coated) was purposely left unbacked. No. 7 was also double-coated and backed.

Up to this writing, plates No. 2, 5, 6 and 7 have been developed. They are remarkably sharp and perfectly defined, and show the prominences and inner corona very beautifully. The polar fans come out magnificently. There is a great deal of beautiful detail in the inner corona that promises to richly repay careful study.

Great care is being exercised in the development of the plates to insure not only detail near the sun but to get also as great extension as possible.

Other instruments used were a 6-inch and a 3½-inch portrait lens, mounted on the equatorial mounting formerly owned by Mr. Burnham and kindly loaned by Professor Comstock.

A 4½-inch Brashear portrait lens, and a very sharp focus (1:1.9). Voightlander lens of 4 inches aperture, were strapped to a fixed post and were used for quick exposures on the corona. Three Sonader lenses, kindly loaned by Mr. Ellerman, were placed on a small heliostat mounting and were given an exposure extending nearly through totality, with three different kinds of plates.

The plates with the small instrument have, in part, been developed. The results show an extension of the corona between 3 and 4 diameters of the moon. The corona, as shown on these plates, is singularly like that of January 1, 1889. There is a broad fish-tailed extension spreading out to the west more than half way to the planet Mercury, which shows on all the plates. A long, thick set, more or less pointed mass, extends to the east for several diameters of the moon. The poles are surmounted with fan-like systems of rays—the whole entirely characteristic of the sunspot minimum corona.

The results with the cœlostat show that it is by far the simplest and best instrument for securing photographs of the corona on a large scale. The results also show that the images could be made very much longer with much gain. An instrument of this kind for photographing the corona could safely be made several hundred feet in length with a corresponding increase of aperture.

G. A. Hill. A few remarks with regard to the observations especially made to determine the position of the moon. In several of the reports the idea is presented that the time predicted from the ephemeris was not accurate. This I think was caused by the position of the observer and his station not being exactly known. At Lawrenceville we had a transit telescope, and we computed the times of contact. The times of the second and third contact agreed within 0.5 second with the ephemeris, and the times of the first and last contact came within 1.9 second. The duration of the totality was exactly given from the ephemeris. At Barnesville we had a clock giving seconds, with the 60th second left out, and the gentleman who took the time there lost his count, omitting that second, so that we were a second late in our time. But this was not the fault of the ephemeris, but of the man who counted on the clock. I was much gratified with the condition of the

sun on the morning of the eclipse. The sun was unusually steady, and we succeeded in obtaining contacts that were quite satisfactory.

M. B. Snyder. I would like to call attention to the prominence on the southwest corner of one of the plates. I observed it visually, and was puzzled by the peculiar extension of that prominence. There was a decided change in its character. At the top there was a gradual fading of the prominence and the corona; and this is brought out by the photographs shown.

Ormond Stone. In the observation of contacts it seems to me that there is a different way from the usual way, and which is preferable to it. The method used by myself in the eclipse of 1878 was used by Mr. Morgan at the present eclipse, at my station. I am not aware that it has been used elsewhere. It gets rid of the difficulty which arises from the fact that, especially at the first contact, we recognize the contact by an indentation. In other words, we do not observe the first contact at all, but we recognize as the first contact a time which is not the first contact. The method which we used was this: A divided scale was placed in the field of the telescope, with 20 divisions on each side, with every fifth division a little longer. This was placed at the proper angle to correspond with the diurnal motion, and carefully focused. When the indentation in the sun's edge had the length of one division a record was made; when it had reached the length of two divisions another record was made and so on through. By examining the report of the eclipse of 1878 you will see how closely the different results agreed. The observations of the present eclipse have not been reduced, but it seems to me that some such method as I have described is far preferable to the method of noting the time when the indentation is large enough to be fairly visible.

Edgar Frisby. The observation of the first contact involves the knowledge of about the position where the contact will occur. Of course it is always a little late, but when the observer knows the position and is looking very closely, the error must be very small. We should expect the last contact to be very close; but I have found, from the experience of three or four observers, that the agreement was not so close as in the first contact. It seems after all to be more difficult to note the exact time of the last contact than of the first.

A Member. With reference to the remarks of Professor Upton, I may say that I had charge of the camera, attached to which was a visual telescope so that we should be able to know just when to get the flashes. The time of the first flash was successful, and I made the exposure and got a number of bright lines. The second exposure was made after about ten seconds, during the totality, and it partially showed the bright lines. The chromospheric rings were not well shown because we had no driving clock. The attempt to get the flash at the last contact was not successful in consequence of the shortening of the time of exposure. After this I made an exposure of 10 seconds every minute, and several plates showed a continuous spectrum. One minute after the third contact the lines were beautifully shown. The cause of the apparent continuous spectrum was perhaps the fact that the background was not sufficiently bright to show dark lines. Possibly they were all there but not thoroughly brought out.

M. B. Snyder. You will remember that a year ago I called attention to the use of the phonograph as a means of recording the time. During this eclipse I have actually tried the phonograph, with results quite satisfactory. We concluded that we might observe the shadow bands, and erecting a screen directly opposite to the sun we ob-

served these bands and recorded the results by the phonograph. In this I was assisted by Mr. Thompson.

Mr. Thompson. At Mr. Snyder's request I will say that having gone to observe the eclipse, I undertook in co-operation with him to watch for the shadow bands. I stood about 20 feet away, and about three minutes before the sun disappeared I noticed very faint streaks. I did not communicate this to Mr. Snyder until they became unmistakable. At that time they were moving, so far as could be determined, at about the rate of a man running, say, 20 or 30 feet per second or perhaps faster than that. The points were curved, and they appeared to be broken into little curves 4 or 5 inches apart. I should think the dark places increased in width as totality approached until the darkness seemed to cover the entire space, and a few seconds before the totality everything had gone into confusion, and there was no definite progress after that. Just after the totality I noticed that the same direction as before was maintained, but that there was another system of bands moving in the opposite direction. Thus there were two sets of bands moving in opposite directions, for a very short time, of which Mr. Snyder has the record in the phonograph. Those that moved in the original direction persisted, fading out gradually in about the same time they took to come on before totality. I think it very likely that the confusion which I noticed just before totality was due to the action of the two systems of bands.

Brown Ayers. As I have never figured in the scientific world as an astronomer, it may have been presumptuous for me to undertake to do so much as I did in the observation of the eclipse. But I realized that my position at New Orleans was near the center of the totality, and there were parties who came to our place to observe the eclipse, but who located on other grounds. The location seemed to be poor, and one or two who thought of coming gave it up. But I thought as we were the only institution of any magnitude on the line of the eclipse it would be absurd if we should allow it to pass without any observation whatever. When we received the little ephemeris sent out by the United States Naval Observatory, we laid our plans as to what we could do with the appliances at hand. The result was that I tried to observe the shadow bands, and to get the contacts as well as possible. That was my program until about ten days before the time of the eclipse; when Chancellor Fullerton of the University of Mississippi, wrote and offered to loan me a fine telescope of 15-inch visual and 9-inch photographic aperture, made by Grubb. I received it gladly, and mounting it prepared to take some photographs with as much accuracy as I could. But the main point I had in view was to get the times of contact. There were different points in the city carefully determined and marked by the Coast Survey, so that I was able to get my position accurately, and I could get Washington time by signals in my observatory. I was able to make a very large number of comparisons to obtain the local time. The plan I adopted was a combination of visual and photographic work. We took the pictures as rapidly as we could feed the plates into the telescope, the position being carefully adjusted by star-trails, and we had a snap shutter automatically recording the time at which each picture was taken. This gave us a large number of photographs of which the time was accurately determined. We were so fortunate as to get a plate showing the Bailly beads, and the next plate was blank; so that we have secured a very accurate observation of the time of the second contact. At the time of the third contact I was peculiarly fortunate; for just as the plate was exposed by the snap shot, there was a

sudden flash, and I think I have the time of that contact photographed pretty closely. In addition to my work, Professor Fullerton, who was within a few feet of my observatory, made visual observations; and I think we can work up some successful results. Instead of the collection of astronomers they had at Waynesborough, we enjoyed isolation, and the eclipse was an old story with us before they could do anything in Georgia or North Carolina. There was one little thing I should like to note, to which my attention was called by Mr. Brashear. It is that one large prominence which on my plates is distinctly turned over, half an hour later in Georgia was standing straight up. This shows the change which had taken place after my observations at New Orleans. We had beautiful weather until the eclipse was over, when it immediately clouded up. We took over 50 photographs; and although we had no astronomers with us, unless we count Professor Fullerton as one, I think we did our best.

Winslow Upton presented a report of shadow bands observed by John Ritchie Jr. of Boston, and also those observed by Edwin F. Sawyer.

W. J. Humphreys. Mr. Ayers states that just as the exposure closed, at the second contact, he looked up and saw the last of the sunlight go; that the totality had not quite begun when this exposure was made. The plates shows not only no dark line spectrum at all, but there were bright lines. This seems to show a very remarkable phenomenon. At the second and third contacts there was a slight depth of solar photosphere and a great deal of halation; but immediately after the third contact there is no halation whatever, and the lines are extremely bright, showing the crescent due to the plate and the chromosphere. I noticed something that seemed to be of the same character. It was $2\frac{1}{2}$ to 3 minutes before the second contact. I was observing with a telescope made up of a field glass and a small reflector, using no slit, and when I began observing I saw the narrow crescent of the sun as a dark crescent in the spectroscope. The form of the lines, instead of being straight, was crescent. Then the bright lines began to encroach upon the dark crescent, and the dark crescent began to shorten and broke up into a number of short crescents, and these came down to a narrow line, not a dark line but a bright line, which continued for nearly a second and then suddenly disappeared, when the whole field was filled with bright lines. The *F* line, at the time of totality, extended around almost the complete circle, perhaps three-quarters of the way round, and at that time there were only a few other lines observable. The magnesium lines showed somewhat. One thing I wish to call attention to is that the photograph shows that at the second exposure there were only bright lines and no dark lines at all. Mr. Gilbert was under the impression that the photosphere had not quite gone. The base of the chromosphere was intensely bright, and this is required for the reversal of the small lines which are produced very close to the photosphere. I also call attention to the fact that where the spectrum was almost continuous there was a very decided difference between the brightness of the bright lines and the dark lines. Very many of the dark lines were reversed, and there was no such phenomenon connected with the bright lines.

W. W. Campbell. This layer which is spoken of is shown in one photograph which was taken before the totality was ended, absolutely that and nothing else, and it extends over 150° on the western side of the sun. Another point that I wish to emphasize is our appreciation of the kindness of the Weather Bureau in sending us bulletins. The Weather Bureau obser-

vations showed that a strip of territory in western Georgia and in eastern Alabama, had the best chance of securing observations of the eclipse. But in fact I think we had the poorest weather. A minute after the eclipse was over the sun was thickly clouded over, a large mass of clouds coming up from the west. In connection with this I will say that a member of our party made it a part of his business to observe the corona, and he found that he could visually observe the corona 70 seconds after the eclipse was over. We thought it was not worth while to take photographs at that time, but we could probably have followed the corona longer except for the clouds which came up and covered the sky at that time. With regard to the green ring in 1898, which was shown very much broken up, indicating great coronal activity there, in this eclipse we had the green material piled up in large masses.

A Member. I was watching the region of F, to learn how to keep the image in the slit. It was not a gradual nor a sudden change, but a flickering one; and it went out a number of seconds after the totality in exactly the same manner that it came in, so far as its brightness was concerned. I am sure that if the best arrangements were made to economize the light, a grating of $21\frac{1}{2}$ inches focal length could be successfully used. The light from that part of the sun which was upon the slit was not more than 1/200th part of that thrown by the cœlostat upon the ruled portion of the grating. If we could in some way economize the slit we might get good results from it.

Mr. Jewell. Probably the flickering spoken of was due to a slight tremulousness of the atmosphere. An interesting thing to observe was the carbon bands, which were exceedingly bright at a distance of 100 or 200 miles above the sun's surface, and which extended out about 400 miles.

OBSERVATIONS OF EROS AT THE COMING OPPOSITION.

Simon Newcomb. Some one has divided astronomers into two classes, those who talk about things to be done, and those who go to work and do them. In the present case I am afraid we shall have to enroll ourselves in the first class, because it is not easy to do anything in this matter, the situation in this country not being favorable to the determination which we have in view. You are doubtless all aware of the great interest attaching to this remarkable asteroid. It may be said to supply us with what we have long been wanting, an object admitting of exact observation, which at proper intervals will come so near the earth that the solar parallax can be determined with greater precision than by any other method. It would hardly be possible to get one more exactly to fill the bill. The minimum distance of Eros from the earth is 0.15 of the distance of the earth from the sun. It follows, therefore, that at certain times it will be about as near to us as observations can advantageously be made. Were it to come very much nearer the additional advantage would be slight, for the reason that the elements of its orbit could hardly be known with sufficient accuracy to give us greater advantage; 0.15 is all that we can ask for, since that distance will diminish the effect of errors of observation by six or seven times. We have made very little progress in this direction for fifty years, and may now hope for something more. Yet in Eros, the Fates or whoever rules our destiny, have supplied us with something very tantalizing. It turns out that the nearest approach of Eros to the earth occurs only on rare occasions. A nearest approach occurred in 1894, and another approach as near as that will not occur until after the middle of the coming century. But an approach as near as can occur in the next twenty years will take

place in the coming autumn, when its minimum distance from the earth will be about 0.3, or twice its absolutely shortest distance. It is very desirable that astronomers who devote their attention to the determination of the solar parallax, should take advantage of this opportunity for we shall have a parallactic displacement of Eros exceeding three times and possibly five times the solar parallax. But there are many drawbacks, the planet is faint; it will probably be, when nearest to us, but little above the ninth magnitude, and it will therefore be scarcely possible from its rapid motion to photograph it, or to ascer- its true position upon the plate at any given time. It is difficult to ascertain just how the observations upon it should be made. With the view of getting the ideas of those who are interested in the matter, and of ascertaining how the stations can best be planned, I have published in the *Astronomical Journal* a paper upon this subject, accompanied by diagrams, to aid in the selection of the observing stations. These diagrams give four projections showing the position of the earth as seen from the direction of Eros, at different periods during the opposition. (A copy of one of the diagrams was placed upon the blackboard.) If we imagine an observer looking down upon the earth from Eros, the relative parallax between any two points upon the earth's surface, will be the distance of those two points as seen by the observer upon the planet. These diagrams therefore show us the advantages and disadvantages of any two stations we may choose. The line of sunset, showing when Eros will be visible to us, will be nearly the same for several nights in succession. Taking for example December 16th; in that part of the earth upon the left of the diagram, it will be day, and observers there cannot see the planet at all. It is only in the remaining region that an observer can see the planet. I will mark upon this portion degrees of latitude, showing the position of different observatories. On the circle of 60° north latitude, Pulkowa and Helsingfors are situated, their position upon the circle depending upon the time of observation. It is evident from the diagram that the parallax at those points will be very great; and those two places have the advantage of being near one end of the base line giving the greatest observable amount of parallax. The asteroid being so far north on December 16th, does not set at all at those places. It is evident that the best corresponding point, giving the longest base line, will be in the portion of the diagram south of the equator. The planet at that time is only visible about 15° south of the equator and the only observatory in that region is at Arequipa. If then observations are made at the Arequipa observatory, for an hour or two, and if observations are also made at Pulkowa or Helsingfors for an hour or two, it will give us the longest possible base line for these observations. Observations may be made at other points, but they will be less available in the determination of the parallax than those made at Pulkowa or Helsingfors and at Arequipa. Another point for consideration is that if the two sets of observations are made at different times, the planet will be moving in the meanwhile. Its motion is very rapid, and an element of uncertainty is introduced by the necessity of determining the amount of this motion. If the observations at Pulkowa or at Helsingfors and at Arequipa are not simultaneous this uncertainty affects the result. The question arises therefore whether simultaneous observations can be obtained at the ends of this base line. To determine that question we have only to imagine a map of the world upon this diagram. As the observer at Eros comes in sight of the oceans and continents of the earth, passing before him, he will see the two stations, at Helsing-

fors and at Arequipa; and observations cannot be simultaneous unless he can see both at once. We see that the conditions are extremely favorable. While Arequipa is passing along in *this* region, near the sunset line, Pulkowa will be passing along in *this* region, near the opposite horizon; so that we may combine observations made at Arequipa early in the evening, near the end of twilight, with those at Pulkowa several hours later in local time, in the latter part of the night, and they will be nearly if not quite simultaneous. We can thus obtain the maximum parallax, which will be 48″, with a little allowance for motion, from the observations of those two stations. Later in the season the Cape of Good Hope will appear above the horizon, and then another combination can be made, the circumstances will be less favorable. We might have observations at Paris, Greenwich, Potsdam, etc., but they will afford less favorable means of comparison than Pulkowa and Arequipa, which give the longest possible base line. As to our own position, we can observe in connection with Arequipa, but not easily; and in connection with the Cape of Good Hope, but when Eros rises with us it will set at the Cape of Good Hope. On the whole we cannot too strongly impress upon Professor Pickering the desirability of getting at Arequipa the best observations that can be made there, for combination with all the other stations. As to the method of making the observations, undoubtedly we must depend upon photography. There is no heliometer at Arequipa, and at the Cape it is not certain that it is available. It will not do to depend upon the heliometer. We must depend upon photography, and it is not easy for one not an expert to make valuable suggestions with regard to that. An additional complication arises from the rapid motion of the planet. It does not appear certain that the planet can be photographed at all so as to be available for this purpose. It must rest in one position long enough to take a picture or there will be no trace of it upon the plate. Professor Pickering says that there will be no difficulty in getting a trail; but that will not answer the purpose. In view of the fact that most of the stars will be brighter than the asteroid, photographs may be taken by focusing upon the planet and taking a trail of the brighter stars. But the difficulty remains of getting the time from the trails. Another difficulty is that we have no first-class photographic apparatus in the equatorial region, available for this purpose. I conclude that the combination of Pulkowa and Helsingfors with Arequipa ought to be our main reliance for obtaining the value of the solar parallax.

G. A. Peters. I should think that owing to the fact that the planet is upon the horizon during the observations for parallax, the light would be much diminished, and that the photographic plan would in that respect suffer a disadvantage in comparison with observations obtained visually. Unless large telescopes are used, and the atmosphere is extremely clear, there is danger that no observations can be obtained in that way.

Professor Newcomb. It is fortunate that at the stations where the planet is near the horizon the heavens are remarkably clear. At Arequipa, Pulkowa and Helsingfors the vapors near the horizon are but a minute fraction of those in the clime of Washington. It is a curious meteorological fact that here the vapor is generally so dense that good seeing near the horizon is much more rare than at the stations selected.

C. C. Abbott. Would it not be well to use isochromatic plates?

Professor Newcomb. That is a detail that we shall probably have to leave to those who take the photographs.

S. J. Brown. I have given this matter

some attention. It seems to me that micrometric observations could be carried on simultaneously, especially in the United States. We have Pulkowa, Strassburg, Charlottesville, Evanston, Madison, Princeton, Washington, and the Lick Observatory, and the Yerkes, where work can be advantageously done with large telescopes. While the factor of the parallax is very much smaller in the case of simultaneous micrometric observations, the main thing seems to be to secure the necessary co-operation, and to prepare a careful plan beforehand. The motion of Eros is so large that the observations will have to be very nearly simultaneous. The difference in the factor for parallax between Pulkowa and Washington is about 1.1. It is not much greater between Pulkowa and the Lick Observatory. It is 1.92 at Charlottesville; 1.26 for the Lick Observatory, 1.13 for Washington, and about the same for Princeton; so that by simultaneous observations between Pulkowa, Strassburg, Greenwich, and the observatories in the United States, we may obtain a parallax of about 15″ between the 1st of October, and January 16th. My impression is that stars should be selected not more than 60″ from the planet, and that they should be compared at nearly the same Greenwich mean time at all the stations. In this way observations may be obtained extremely valuable for determining the parallax. I am not certain but that the planet can be photographed to advantage: but it would require a long time, and the motion of the planet would be uncertain; so that the advantage I think would be outweighed by the accuracy with which micrometric observations could be made; and if referred to the same stars, eliminating the motion of the planet, we might expect precision. The position of the planet can be ascertained closely enough, and its parallax known so nearly that we can measure it to advantage in telescopes varying from 16 to 40 inches in aperture in the United States. I had started to draw up a letter to be sent to the directors of the observatories of Pulkowa, Strassburg, and Greenwich in Europe, and in this country at Charlottesville, Princeton, Madison, the Yerkes and the Lick, asking that co-operative work might be carried on, and asking for a selection of the stars. If we assume 60″ for the greatest distance from the planet, and a parallax varying from 15″ to 30″, this will give a distance from 60″ to 90″; and you can go down to the 10th magnitude, the magnitude of the planet, and find in the field a number of suitable stars, which should be symmetrically situated in rectangular co-ordinates. In this way it seems to me that we can obtain from these large telescopes observations of as great accuracy as those obtained by the photograph, although the distance may be twice as great and the factor twice as great. I do not see that anything practical can be done by this association. It must remain for some one to take the initiative; and there ought to be no difficulty in getting suggestions to perfect such a simple plan, such as may result in saving the day for the observations of Eros. Photographic observations would have to be confined largely to Pulkowa, Berlin, or Potsdam, and Arequipa, the Cape of Good Hope will not come in until along towards the end of the opposition. It seems to me therefore that micrometric observation, under these circumstances will be a matter of considerable importance.

Professor Newcomb. We want the stars as nearly of the same magnitude as possible. In a space 60′ square, the mean distance of the stars would be about 6′. The planet moves about 2″ of arc in a minute. An observer making a careful micrometric measure must take at least ten or twenty seconds to make up his mind about the bisection. During 10 seconds the planet will have moved over a space of $\frac{1}{3}''$ of arc, a quantity not appre-

ciable but still large enough to leave open the question whereabouts during that period of 10 or 20 seconds occupied in making the bisection, the planet actually was at the time assumed for the bisection. There arises an uncertainty which may be systematic. It may be different for different observers and for different places. This uncertainty seems to preclude dependence upon the process of micrometric measurement. In the case of the satellites of Saturn we know that results are obtained differing enormously.

S. J. Brown. And for the satellite of Neptune the difference is still greater.

Professor Newcomb. There are some systematic differences attending micrometric observations, but these can be diminished if we take our observations to avoid them. We have the method of difference of right ascension and declination, when we take the right ascension entirely free from the declination, taking the right ascension at the time of passing a certain thread. If the star is chosen of about the same magnitude as the planet, it will be free from one source of systematic error, but there may be a great many others. If each set requires a minute or two, and if two threads are used, you may make 60 comparisons in an hour, which I think will be more than can be done with the micrometer. There is then much room for systematic error.

G. W. Hough. I think the most accurate work can be done with the micrometer. With an $18\frac{1}{2}$-inch telescope there is no trouble in seeing an 11th or 12th magnitude star; and it is only necessary to get the time of the bisection and the position angle and distance. The time can be known within a few seconds, and ten observations can be made as readily as one. So we can get a final result free from systematic error.

Simon Newcomb. That may be quite possible for a 12th magnitude star.

G. W. Hough. The method of differences of right ascension and declination I use very often; but then we must depend upon the stability of the telescope. It may require $1\frac{1}{2}$ to 2 minutes to pass from the star to the planet, and there is a liability to error from the action of the wind or from other causes. In photographic work the star will be near the horizon at the time required for its observation, and the time required will be very long, especially if we use isometric or other such plates; and that will be a great objection.

W. W. Campbell. It seems to me that both the methods suggested should be submitted to actual trial before the entire campaign is based upon either method. It ought to be possible by means of known asteroids moving rapidly to obtain photographs, by allowing the stars to trail or the asteroid to trail, and we could learn with what accuracy the position of the asteroid can be obtained upon a certain plate. Furthermore it will be easy to test the method of observing faint stars near the asteroid. In large telescopes it will be possible either to get a star of the 12th or 13th magnitude within 1′ of arc, and by selecting stars in different directions from the asteroid it will be possible to eliminate practically all the personal element entering into the question. In my experience in micrometric measures there is never an uncertainty greater than 2 or 3 seconds as to the exact instant of bisection, and the motion of the planet in that time, for that particular observation, will be small as compared with the accidental error probably made. If we take ten observations together the motion of the planet will be practically unimportant. My main point is that the method fixed upon should be actually submitted to test before the campaign is started. It would be a serious matter if the campaign should be carried on, and afterwards it should be ascertained that in consequence of some oversight the results are of very little importance.

G. C. Comstock. If 12th magnitude stars are to be used in the comparisons, can provision be made to secure the observation of the same stars at the different stations?

W. W. Campbell. A photographic plate can be secured from which the accurate positions of these stars can be ascertained.

G. C. Comstock. Is there time for this before the campaign begins?

W. W. Campbell. I think there should be a carefully prepared plan.

S. J. Brown. In relation to the observation of differences of right ascension and declination, all the observations will have to be made before we can determine the corrections, because the changes of right ascension and declination will not be in proportion to the elapsed time in consequence of the rapid motion of the asteroid. We need to eliminate the systematic errors. You mark your time, and your measurement does not correspond with the time you have marked. That leads to a systematic error. If we can eliminate this I think that observations of great accuracy can be made. In this case it is proposed to select stars which shall not be more than 30 seconds in time away from the planet. In micrometric measures the suggestion is 60″, which added to the displacement of the planet amounts to 90″. Above that distance there would be a rapid increase of probable errors. The main difficulty appears to be in the selection of stars down to the 11th or 12th magnitude.

W. W. Campbell. It seems to me that the method of observation of transits is objectionable. With the equatorial telescope there is a tremendous personal equation in the observation of stars and comets. Undoubtedly there will be a similar personal equation in the observation by transits of an asteroid and a faint comparison star differing 3 or 4 magnitudes from the asteroid. There are many things that may occur; the refraction may be changed, or the position of the instrument may be changed if we wait a minute or two for the comparison star to come along. Our experience in the observation of comets shows that very much better observations can be secured if we throw overboard the method of observing differences of right ascension and declination.

G. W. Hough. I disagree as to the time required for the observations, and I have made thousands of observations of differences of right ascension and declination. But one measure of the position angle is better than anything you can get from differences of right ascension and declination.

S. J. Brown. We have to shift the micrometer 90° in taking the position angle. Suppose the planet to move 1″ per minute, the change in the angle is not proportional to the time, and the change in the distance is not proportional to the time. I have come to the conclusion that differences of right ascension and declination can be determined with greater accuracy than differences of position angle and distance. One point we wish to agree upon is a uniform method of measurement. Observations cannot well be compared if one observation is made upon one plan and another observation upon a different plan. I have no doubt that the difference in magnitude is a very important factor in determining the position. In my observations of Titan I reduced my observations by the method of least squares, and all the equations gave positive residuals except one; and that could only be explained upon the theory of a variable personal equation from difference of magnitude. Even with faint stars it would be difficult to use differences of right ascension observed with the chronograph or eye and ear. I think we ought to have the experience of those who have had experience with stars of the magnitude to be used as to their method of treating them. The photographic method I think is subject to

special dangers on account of the low altitude of the asteroid in the southern hemisphere.

W. W. Campbell. The base line which Professor Newcomb has indicated has a great extent, but as to successful working much depends upon a wise compromise which ought to be considered. At the expense of longer observation the observers are going to work near the horizon, where the refraction will be more uncertain. Furthermore the image to be measured will not in all cases be satisfactory on account of the vapors rising near the horizon. What amount of difference in the base line will outweigh the differences in the atmosphere we shall have to compete with? It appears to me that a base line which gives only two-thirds of the parallax may be more accurate than one which gives five-sixths, in consequence of the better conditions under which the observations may be made.

G. C. Comstock. We should not be misled by the drawing upon the blackboard, because it is not drawn to scale. Taking the indicated positions, we shall not bring our observations very near the horizon. I agree that we should not seek the maximum possible base line, there are other considerations in the problem, but we should get the longest base line we can without encroaching upon the horizon, and Pulkowa and Arequipa do not crowd the observations upon the horizon.

S. J. Brown. We have to take the telescopes as they can be found. At Pulkowa they have an excellent telescope, and at Arequipa also; and they are of identical power. These two stations are fixed, therefore, by the facts of the case. And so the Cape of Good Hope will be fixed as another station. The photographic program is fixed. I think the matter will be settled according to the circumstances as they exist now. We have three photographic telescopes of 13-inch aperture distributed very favorably in the southern hemisphere, and three or four in the northern hemisphere. The micrometric problem is one which I think the astronomers of America can agree to co-operate in, and I think we ought to have considerable influence in inducing Strassburg and Greenwich to co-operate with us. If we can secure the agreement of astronomers in this country and send word to those in Europe before October 1st, we may possibly save the situation, and perhaps produce results comparable in accuracy with those from the photographic plates. The planet may be observed at Pulkowa and Washington from October 1st to January 16th, the observations at Pulkowa extending to an hour angle of 6 or 7 hours. Then the planet would have an altitude of 30°; and the factor can be extended from 15″ to 30″ or even 35″. But I have been unwilling to write to foreigners until I could ascertain the opinions of astronomers here.

Winslow Upton. I think if Professor Pickering were here he would assure us that Arequipa is remarkably well situated to adapt it for a southern station. The condition of the climate and the atmosphere is favorable, and it has an elevation of 8000 feet above the level of the sea, ensuring good definition. Again, the twilight there is always short. We can be perfectly sure that Professor Pickering will use the facilities of the situation to the utmost. But there is a difficulty from the unknown law of refraction. The discussion of the observations will take the differential form, but the law is not known at all. These considerations do not apply to Pulkowa, but there the planet will not be low.

F. L. Chase. With regard to diurnal motion, will it be possible to eliminate the motion of the planet?

S. J. Brown. I think the micrometric method in that respect out of the question. The motion of the planet is about 10′ in 10

hours, and it would be difficult in a large telescope to measure that arc of 10′. We find that with the great telescope at Washington we are limited to 4′ or 5′. Beyond that we do not get good results. You cannot make the observations without shifting the eyepiece; and I have made up my mind that for the purposes of micrometric observation the diurnal plan would be out of the question.

J. G. Hagen. Referring to the photographic method, there is a difficulty which has not been discussed. The motion of the planet, in taking a photograph, which may require an hour or more, will produce a trail. There is an opportunity to choose between allowing the planet to trail, and allowing the stars to trail. Perhaps the same object, of securing an accurate position for the planet upon the plate, can be attained by interruption of the exposure. The time of the motion can be known within ten seconds; and while the planet, of the 9th magnitude, may be obtained upon the plate, the bright stars may be obtained in a series of dots, each exposure giving an independent determination. Whether these interruptions should be made once in ten seconds or once in a minute is a question for experiment. I merely make the suggestion of a method that may remove the difficulty.

Adjourned.

GEO. C. COMSTOCK,
Secretary.

WASHBURN OBSERVATORY.

SCIENTIFIC BOOKS.

Curing and Fermentation of Cigar Leaf Tobacco. By OSCAR LOEW. Report No. 59, U. S. Department of Agriculture. 1899. Pp. 34.

Physiological Studies of Connecticut Leaf Tobacco. By OSCAR LOEW. Report No. 65, U. S. Department of Agriculture. 1900. Pp. 57.

The great and growing importance of the tobacco industry in the United States has led the Department of Agriculture to undertake an extensive series of investigations, covering the mapping of areas of soil suitable for raising tobacco, studies in fermentation, improvements in breeding and selection, the conditions of growth and manipulation in foreign countries and the question of supplying tobacco to foreign markets. Dr. Loew has been detailed to carry out the chemical part of these researches, and the above-mentioned documents set forth the results thus far obtained by him and by others, and contain an abundance of material, both of scientific interest and practical importance.

From the consumer's standpoint essential constituents of tobacco are: (1) nicotine; (2) certain compounds, the chemical nature of which is wholly unknown, which impart to the leaf its acceptable flavor or aroma, and which differ in quality and quantity in different grades of tobacco. It is upon the latter, rather than upon the nicotine, that the commercial value of the prepared leaf mainly depends. The amount of nicotine is largest in the fresh leaf and undergoes marked diminution during fermentation, while the aromata are developed during the processes to which the tobacco is subjected. There is no apparent relation between the color, aroma and amount of nicotine.

The preparation of tobacco comprises two stages, curing and fermentation or 'sweating,' sometimes supplemented by aging or 'cold sweating.' During the earlier part of the curing stage the cells are still alive, and the resulting changes are physiological, embracing among others the transformation of starch into sugar, and the partial respiratory consumption of the latter, or its transference to another part of the leaf and reconversion into starch. After the death of the cells, the proteolytic and oxidizing enzymes attack much of the protein, fat and tannin, and give rise to changes of color and flavor. The curing stage is followed by one of fermentation, which goes on under the influence of air and moisture, and which is accompanied by a marked rise of temperature. During this stage there is a notable decrease of nicotine, but an improvement of flavor and aroma. The fermentation was ascribed by Suchsland to the action of bacteria, and he attempted to impart to German tobacco the peculiar Havana flavor